

AD-A087 689 CALIFORNIA UNIV BERKELEY STATISTICAL LAB

F/G 4/2

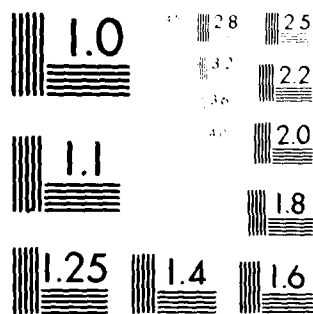
LETTER TO THE EDITOR OF COMMUNICATIONS IN STATISTICS AND COPIES--ETC(U)

N00014-75-C-0159

NL

UNCLASSIFIED

END
DATE
FILMED
9-80
DTIC



MICROCOPY RESOLUTION TEST CHART
 NATIONAL BUREAU OF STANDARDS-1963-A

ADA 087689

DISCLAIMER NOTICE

**THIS DOCUMENT IS BEST QUALITY
PRACTICABLE. THE COPY FURNISHED
TO DTIC CONTAINED A SIGNIFICANT
NUMBER OF PAGES WHICH DO NOT
REPRODUCE LEGIBLY.**

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

REPORT DOCUMENTATION PAGE		READ INSTRUCTIONS BEFORE COMPLETING FORM
1. REPORT NUMBER ONR 80-04	2. GOVT ACCESSION NO. AD-A087689	3. RECIPIENT'S CATALOG NUMBER
4. TITLE (and Subtitle) Letter to the Editor of COMMUNICATIONS IN STATISTICS and Copies of Documenting Correspondence.	5. TYPE OF REPORT & PERIOD COVERED 9 Scientific rept	
7. AUTHOR(s) Jerzy Neyman	6. PERFORMING ORG. REPORT NUMBER ONR 80-04	
3. PERFORMING ORGANIZATION NAME AND ADDRESS Statistical Laboratory University of California Berkeley, California 94720	8. CONTRACT OR GRANT NUMBER(s) ONR N000 14-75-C-0159	
11. CONTROLLING OFFICE NAME AND ADDRESS Office of Naval Research Washington, D.C. 20014	10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS	
14. MONITORING AGENCY NAME & ADDRESS (if different from Controlling Office)	12. REPORT DATE June 1980	
	13. NUMBER OF PAGES 47	
	15. SECURITY CLASS. (of this report) Unclassified	
	15a. DECLASSIFICATION/DOWNGRADING SCHEDULE	
16. DISTRIBUTION STATEMENT (of this Report) This document has been approved for public release; its distribution is unlimited.		
17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)		
18. SUPPLEMENTARY NOTES		
19. KEY WORDS (Continue on reverse side if necessary and identify by block number) Experiments: Israeli, Tasmania, Climax.		
20. ABSTRACT (Continue on reverse side if necessary and identify by block number) The LETTER refers to controversies about weather modification experiments. The letter begins by recommending that the several articles, termed "Rejoinders," submitted for publication in <u>Communications in Statistics</u> , be published in the same issue of the journal, accompanied by the present LETTER. The suggestion is that such "compact" publication of the whole material would help the reader to form a sound judgement on the controversies. Some of the assertions made in the LETTER need documentation too voluminous to be printed in		

DD FORM 1473 EDITION OF 1 NOV 65 IS OBSOLETE

UNCLASSIFIED

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

333100

20. ABSTRACT (CONT.)

the letter. The present Technical Report ends with xerox copies of relevant correspondence.

Accession For	
NTIS GRA&I	<input checked="checked" type="checkbox"/>
DDC TAB	<input type="checkbox"/>
Unannounced	<input type="checkbox"/>
Justification	<input type="checkbox"/>
By	
Distribution	
Availability	
Availability or	
Availability	
Dist	23
A	CP

UNCLASSIFIED

LETTER TO THE EDITOR OF
COMMUNICATIONS IN STATISTICS
AND COPIES OF DOCUMENTING CORRESPONDENCE

To: Professor Donald B. Owen, Editor of Communications in
Statistics

From: J. Neyman

It is a pleasure to see my article "Comments on the Special Issue of Communications in Statistics Concerned with Weather Modification Experiments" just published in your issue A9(9), 965-992 (1980) ["Comments" for short]. I am grateful for your sending me the texts of four "Rejoinders" submitted to you by (1) K. Ruben Gabriel, (2) E.J. Smith, (3) A.J. Miller et al, and (4) Paul W. Mielke, Jr.

My "Comments" end with the following passage:

It seems to me that the above experience of Battan, as well as that of the Tasmania experiment, should not be prevented from reaching the Federal Government and the public at large. If they are broadly known, remedial studies are likely to follow, leading to progress in science.

A compact publication of all the four Rejoinders, some of them spectacular, accompanied by the present letter, all in the same issue of your journal, may well become an important contribution to the same purpose: PROGRESS IN SCIENCE. Here are some details.

I. Rejoinder of Professor K. Ruben Gabriel. This Rejoinder appears spectacular by misrepresentations it contains. Its general unreliability is well illustrated as follows.

Protesting my description of the Israeli experiment as described in his own paper (Gabriel, 1967a) as "unprecedented," Professor Gabriel contends that I apply this description to his randomization. The last paragraph of Professor Gabriel's Section 2 is

illustrative. It reads:

Neyman (1980a) refers to this analysis as "unprecedented." I wonder. I can hardly claim credit for being the first to run an analysis by mimicking the ORIGINAL EXPERIMENTAL RANDOMIZATION. Cox and Kempthorne (1963) have argued for doing analysis this way, and so have others. Nor is it unprecedented that the evaluation of an experiment adheres to the same definition of units that was used in allocating the treatments and applying them. (Emphasis added.)

This colorful passage is an elaborate misrepresentation of facts. The description "unprecedented" used by me applied not to the "original experimental randomization" but to the changes in what Gabriel calls units of time. (See his Rejoinder.) First (up to May 11, 1961) the "unit of time" was from 8am to 8am. Next (July 11, 1961 through August 1, 1964) the unit was from 8pm to 8pm. Finally (September 1, 1964 through April 15, 1965) the unit was from 8am to 8am. Apart from the description of the 8pm "cutoff" unit as "unconventional," the early paper (Gabriel, 1967a) does not contain any explanation. Explanations in the Rejoinder (see Appendix I), in such terms as the inconveniences of measuring the rainfall with the 8pm cutoff, appeared later. Appendix I of the Rejoinder indicates 1969.

Both in his article (Gabriel, 1967a) and in his Rejoinder, Professor Gabriel emphasizes that the whole design was determined BEFORE the experiment started. Does this apply to the dates of the "unprecedented" changes in the "cutoffs"? The very explanation in terms of "inconveniences" in measuring nighttime rainfall contradicts this idea.

For convenience of verification in the Editorial Office, I enclose a xerox copy of Professor Gabriel's article referred to as Gabriel, 1967a.

II. Rejoinders on the Tasmania Experiment. There are two Rejoinders concerned with my "Comments" on the Tasmania experiment. They are authored by Dr. E.J. Smith alone and also by himself in the company of Dr's. A.J. Miller, D.E. Shaw and L.G. Veitch. There is a degree of interrelationship between these Rejoinders and it seems

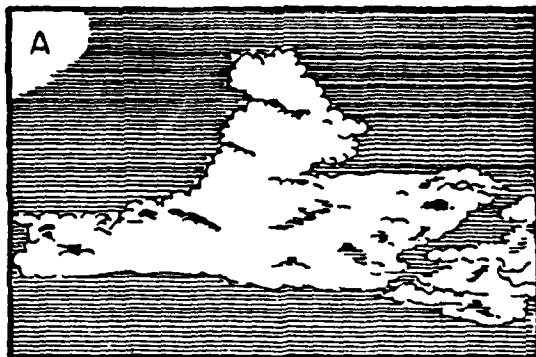
expedient to discuss them jointly.

(i) Both Rejoinders exhibit interest in the hypothetical mechanism whereby the local seeding of clouds can have strong effects on precipitation in far-away areas. A hypothetical mechanism of this kind, which I find interesting, is due to experimenting meteorologists J. Simpson and A.S. Dennis [NOAA Technical Memorandum, ERL OD-14 (Department of Commerce, Boulder, CO, 1972), pp. 1-148]. The mechanism is formulated in terms of "precipitation break" and "orphan anvil." Theoretical discussions are accompanied by nice photographs of clouds in question. One photograph illustrates the "cut-off tower regime which often follows dynamic seeding of a single cumulus." The following illustration is redrawn from this photograph. The "orphan anvil" is described as a big entity at high levels capable to "kill many cumuli" over very large areas.

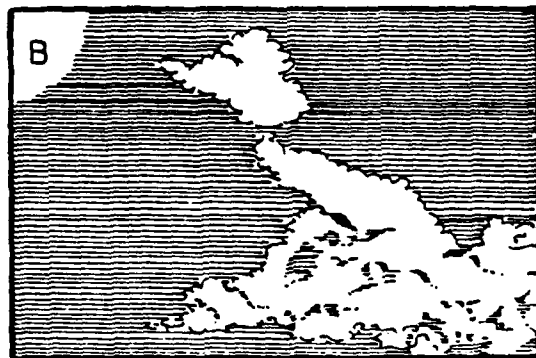
(ii) If the readers of the present letter to the Editor glance at the relevant parts of my "Comments," they will see that, generally, they are complimentary. In particular, I am appreciative of Dr. Smith's explanations such as "both the statistician and experimenter are searching for truth as to what the cloud seeding does," etc. However, this is not to say that I find no criticisms either of the design or performance of the Tasmania experiment. Also, the two Rejoinders show that there are some misunderstandings.

Dr. Smith dislikes my term "apparent effect," and my Figures 1 and 2, presuming that I consider it established that the calculated deficiencies of seed period rainfall ARE DUE TO SEEDING. In actual fact, the purpose of Figure 1 was to see whether the available precipitation data (ALL those published) support the hypothesis that the seeding over the target does not affect the precipitation in the "adjoining" areas. My comment reads:

What is convincing and what is not is a subjective matter. In my own opinion, Figure 1 fails to support the assumption that seeding over the target does not affect the rain over the North Control. If anything, it seems to support the idea that seeding by methods comparable to those in Tasmania can have far-away effects that are stronger than those in the target.



PANEL A: Cumulus at time of seeding.



PANEL B: Same cumulus 10 minutes after seeding.



PANEL C: Same cloud, split into two parts,
18 minutes later.

As to my Figure 2, it was constructed and published in order to avoid possible accusations that I fail to take into account ALL THE PUBLISHED DATA.

This attitude parallels that of Dr. Smith in his Rejoinder, "...Otherwise the analyst may be suspected of choosing an analytic method which gives the results he wants." My own comment on Figure 2 discounts its value.

In the above connection, I note the explanation in Dr. Smith's Rejoinder why the Eastern Subsidiary Area has not been used as one of the controls. Dr. Smith's explanation reads: "We did not use the eastern subsidiary area as a control BECAUSE IT WAS OFTEN DOWNWIND FROM THE TARGET." Emphasis added.

This explanation illustrates Dr. Smith's awareness that seeding over the target CAN AFFECT precipitation in adjoining areas. The area in question had a substantial number of gages. The original report on the Tasmania experiment contains data for this area and our calculations show that the average seed period precipitation was less than that without seeding and that the deficiency amounted to 20% of the latter. When did Dr. Smith decide not to use the eastern subsidiary area? Before or after the precipitation data for that area became available?

As mentioned above, I am appreciative of Dr. Smith's attitude: "Both the statistician and the experimenter are searching for the truth..." Also I am appreciative of the totality of reports on the Tasmania experiment. My criticism of its design and of the performance follows.

(a) The experiment was designed and performed in paired "periods" the length of which varied from 10 to 18 days. I think that this choice was unfortunate. Our atmosphere is affected by a periodicity due to sun. The 24 hr periodicity, with days and nights, has strong influence on temperature, pressure, winds, etc., and also on precipitation. Thus the preferred unit of ran-

domization, etc., is a "day".

(b) The process of randomization is most important and ought to be described in the reports on the experiment. A brief description "at random" is not enough. My preferred methodology is through the use of a proper "random number generator."

(c) As described in the articles on the Tasmania experiment, including the present Rejoinder, the statistical methodology used is simplistic and is open to suspicion that it may have been selected so as to reach pleasing conclusions.

The Tasmania experiment was planned in 1964 while we are now in 1980. Thus the criticisms (a) and (b) do not apply to Dr. E. J. Smith. On the other hand, I disagree with his opinion that our findings for Switzerland and Arizona are irrelevant for the Tasmania experiment. Quite the contrary. If indications of a particular unexpected phenomenon are noticed in several independent experiments performed in widely different conditions, then this is a strong suggestion that the phenomenon is a real atmospheric phenomenon and that the findings do not reflect any kind of local "artifact." The phenomenon in question is that "local" cloud seeding has far away effects much stronger than those in the intended target. To my regret, indications of this phenomenon in the Tasmania experiment are much weaker than they are for the experiments in Switzerland and Arizona. For quite some time our Berkeley group thought that the same phenomenon can be documented for the White-top experiment. To our regret, closer analysis caused us to abandon this hope.

III. The Climax Experiment. The purpose of this section is to explain the reasons for my thinking that the Climax experiment deserves a careful interdisciplinary reevaluation.

A visit to Professor Grant's High Altitude Observatory and some discussions impressed me most favorably. I became acquainted with several kinds of observations and measurements, some rather sophisticated, motivated by the obvious DESIRE TO UNDERSTAND the

complex atmospheric phenomena going on high above the sea level. This contrasted with the many publications TRYING TO PROVE some preconceived idea. The literature on cloud seeding is enormous and I may well have overlooked some relevant publications. But I am describing my own impressions and thinking.

My second reason for advocating a reevaluation of the Climax experiment is connected with Professor Mielke's "Another Response to Professor Neyman" indicating (1) the availability of "basic data on Climax I and II" and their wide use by many investigators. I am uneasy about these data and feel obligated to describe certain developments that, to my knowledge, were not published before.

Professor Grant appeared appreciative of my interest in his research and suggested a cooperative effort. He took the initiative to request the National Science Foundation for support of our work in Berkeley related to his experiment. He was successful and we were to receive some funds. The plans included visits to Berkeley of Professor Grant, possibly accompanied by Professor Mielke, and a delivery to us of some relevant data. Then, suddenly, a change in plans occurred and we had a visit of Professor Mielke alone who brought us a computer print-out, with a number of numerical entries, but with some spots filled by symbolic 99999 replacing a number that the programmer did not want us to see, even though the print-out did not show the relevant dates! Obviously, the print-out could not be used in any study.

The subsequent, somewhat voluminous correspondence revealed that the non-delivery of the promised data was motivated by the fear that we shall publish something ahead of the authors from Fort Collins. The result was that our planned cooperation was cancelled and I had to inform the National Science Foundation that the requested funds will not be needed.

The whole incident is unbelievable and the relevant correspondence is too voluminous to be published with this letter. However, Xerox copies of this correspondence will be delivered to the Editorial Office of Communications in Statistics.

In my paper referred to by Professor Mielke as Neyman J. (1977) it is shown that the experimental days of Climax I included in five consecutive evaluations varied. The numbers of these days varied also, first 279, next 252, then 213, then 251 and finally 251, but the days were different. This concludes my arguments that Climax I deserves a reevaluation.

VOLUMINOUS
CORRESPONDENCE
FOLLOWS

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

November 17, 1967

See next Page

Dr. J. Neyman
and
Dr. Elizabeth Scott
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Drs. Neyman and Scott:

Your participation in the Bureau of Reclamation Planning Session was a stimulating aspect of that program. There is one aspect of the discussions that I would like to explore further with you. On the last day we had a floor discussion about the "state-of-the-art." I believe that there was some misunderstanding on this since my paper discussing this was presented on the first day of the conference before your arrival. I am attaching a preliminary draft of my paper as was presented. Please note page 2 for the "state-of-the-art" comment.

It appears to me that this is directly in line with your findings-- namely that in certain cases changes in precipitation have been indicated from weather modification efforts, but that the processes by which they have taken place need elucidation before we can accept them without qualification.

Our Climax random seeding project has as specific objectives the description and optimization of seeding affects. The design used was developed back in 1960 before regular professional statistical advice, such as Dr. Paul Mielke now provides, were available to the project. Many of the statistical aspects of the program were consequently developed from your suggestions available in publications and papers.

I believe that it has been a good and worthwhile experiment that is

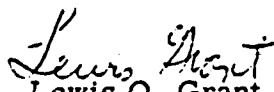
Drs. Neyman and Scott
November 17, 1967
Page 2

providing us with the information that seeding can affect precipitation and that the effects can vary according to existing weather conditions. Dr. Mielke and I have nearly completed a paper that we will be submitting to the J. of Atmos. Sci. in the near future. We are currently checking a second independent sample of nearly 300 cases against the findings with the first sample of nearly 300. We will send you a copy of this as soon as it is completed. I am sending now under separate cover a master's thesis by Mr. Chappel dealing with the effects of seeding under different weather situations.

It does appear to us that emphasis needs to be placed not only on the overall evaluation of weather modification, but particularly on the description of conditions that lead to differing effects. Dr. Mielke and I are starting on modifications in our experimental design to increase its strength in accomplishing these objectives. The primary purpose of this letter is to ask if the two of you would consent to work with us in the further development and refinement of the design that would have these specific objectives. This could be on a cooperative basis between our respective groups or on the basis of private consultations with you.

I appreciate that you have a very heavy work load. We are hopeful that you will agree to undertake this effort since it is obvious that you are quite interested in the general subject and I believe you will agree there is a need for an experimental design of this nature.

Sincerely yours,


Lewis O. Grant
Associate Professor

LOG/dw

Enclosure Designing Programs Involving Ground-Based Seeding
(Preliminary Draft)

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

20 November 1967

Professor Lewis O. Grant
Department of Atmospheric Sciences
Colorado State University
Fort Collins, Colorado 80521

Dear Professor Grant:

This is in reply to your letter of November 17th, which gave us great pleasure. We have read your paper on "Designing programs involving ground-based seeding" and, in particular, your "state-of-the-art" comment. We find that, while your views and ours developed independently and with entirely different backgrounds, they coincide to a remarkable degree.

The subject of study you indicate, the identification of conditions that lead to differing effects of seeding, is exactly the one that we advocated in several of our papers. Thus, it is a pleasure to accept your invitation to cooperate with you and with Dr. Mielke in the work you are conducting.

The first step in this direction must be the establishment of a reasonable contact: you should be informed of what we are doing and vice versa. This implies the necessity of visits. Academic duties will keep us in Berkeley up to December 14th. However, particularly over week ends and on week days after December 8th, it will be convenient to have conferences with you on our Campus. What about coming over for a few days? Then, on December 14th, or later, we would like to pay you a visit in order to gain the feel of the local situation. To begin with this could be arranged using funds that are currently available under our respective projects. Later on, if need be, some extra arrangements might be contemplated.

Professor Lewis O. Grant

-2-

20 November 1967

To be quite specific: what about you and Dr. Mielke coming to Berkeley in the afternoon of Friday, December 1st and spending the week end with us? Please let us know the time and flight number of your plane and whether you want hotel reservations.

Yours sincerely,

J. Neyman

JN/jg

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

December 1, 1967

Dr. J. Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

Dr. Mielke and I are extremely pleased at your willingness to work with us on the problem of design for better defining conditions that lead to differing effects from seeding.

We are both highly over-committed during the next several weeks. Colorado State University is on a quarter system and our quarter ends on December 15. Our most intensive field effort at Climax is planned each year for the week after the fall quarter and just before Christmas. This is the only week of the winter that all of our people can participate in the field program simultaneously. Consequently, in addition to finishing up the quarter, we are deeply involved in getting ready for this special field effort. This makes it very difficult to consider leaving Fort Collins at this time.

I would like to propose the following: Could you visit Colorado as our guests right after the finish of your academic schedule on the 14th or 15th? We could meet you in Denver and then spend a day or two here in Fort Collins establishing contact and going over the general program. We could then all go to our Climax site in the mountains for a day or two and you could see firsthand the experimental setup. You would also, I believe, find it a very interesting and beautiful trip into the Colorado Rockies at that season. If any of your group has an interest, they could then visit one or more of the many beautiful ski areas in the immediate vicinity. I will be staying on at the mountain site until just before Christmas. Paul will, however, be leaving for Minnesota around the 19th.

Dr. J. Neyman
December 1, 1967
Page 2

Paul and I could visit you in Berkeley during January or February,
more or less at your convenience.

Sincerely yours,

Lew Grant
Lewis O. Grant
Associate Professor

LOG/dw

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

4 December 1967

Professor Lewis O. Grant
Department of Atmospheric Science
Colorado State University
Fort Collins, Colorado 80521

Dear Professor Grant:

Many thanks for your letter of November 1st. We regret that you and Dr. Minsch were not able to come to Berkeley next week end, but we understand the difficulty and will be expecting you in Berkeley some time early next year. Our own Christmas vacation for the current quarter will be over on December 15th. On the next day in the morning we shall be able to embark on the trip to Fort Collins. In fact, we now hold tentative reservations for United flight 228, expected to leave Denver at 1:48 p.m., December 15. We hope that this time will be convenient for you.

It will be a great pleasure for us to spend some time with you at Fort Collins and later to visit your Climax site. Our time is cut up with various commitments, presumably to the same extent as is yours, and we would like to return to Berkeley on December 18th, by United flight 265, taking off at Denver at 6:30 p.m. Please let us know whether this will be feasible and convenient. If not, we shall try to change our reservations.

Many thanks for suggesting that we be your guests. However, this will be a business trip and I think we should pay our own expenses. On the other hand, both Miss Scott and I will be grateful if you would make hotel reservations for us.

Looking forward to a very pleasurable and instructive visit.

Sincerely yours,

J. Neyman

JN/jg

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

December 7, 1967

Dr. J. Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

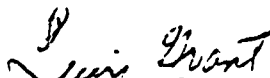
Dear Dr. Neyman:

We are pleased to hear that you will be able to visit Fort Collins and Climax starting December 15. We will plan to meet your United Flight #228 at 1:48 p.m. We are arranging our schedules accordingly. We would tentatively propose that we spend Friday afternoon and Saturday here in Fort Collins, visit Climax on Sunday, and spend Monday here in Fort Collins. Depending on your interests and the progress of our discussions, we would have the flexibility of leaving Saturday afternoon for Climax or returning on Monday morning.

You should plan on bringing heavy clothing for the field trip. Daytime temperatures at Climax at this time of year average in the 15° to 25° F range with nighttime temperature around 0° F or below. Head, hand, and foot gear is very important. We can arrange for special field clothing if you do not have suitable items. We do have heated facilities both at the base and top of the mountain so should never have extended exposure outside.

We are looking forward to your visit next weekend.

Sincerely yours,


Lewis O. Grant
Associate Professor

LOG/dw

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

11 December 1967

Professor Lewis O. Grant
Department of Atmospheric Science
Colorado State University
Fort Collins, Colorado 80521

Dear Professor Grant:

Many thanks for yours of December 7th. I have made tentative inquiries about the cost of warm clothing and found the prospect encouraging. Somehow I shall have to do with my existing clothes. Also, I must tell you that I have some trouble in my legs and, therefore, walking more than trivial distances creates a problem for me.

I would rather think that your original plan to stay mostly at the Center and to pay a day visit to Climax is a good one. In your laboratory you are likely to have the main and the various data that may come under the discussion.

Looking forward to the very interesting meeting.

Yours sincerely,

J. Neyman

JN/jg

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

20 December 1967

Professor Lewis Grant
Professor Paul Mielke
Colorado State University
Fort Collins, Colorado 80521

Gentlemen:

This is to thank you for your charming hospitality
Miss Scott and I experienced last week end at Fort Collins
and to summarize very briefly our discussions.

1. We are impressed by what you are doing in connection
with the Climax experiment and would like to cooperate.

2. As agreed, Lou will write a letter to the NSF
requesting a \$5000.00 increase in his current budget to be
used for a subcontract to cover expenses on our work in Berkeley
over the next couple of months or so.

3. Simultaneously we will prepare an independent project
application to the NSF to cover our cooperating effort over a
more extended period. The two projects, yours and ours, would
be cooperating but funded separately.

4. We shall be interested to see your proposal to the
Bureau of Reclamation with the prospect of some sort of cooperation.

5. In connection with the proposed joint study we hope to
receive your observational data. As of now, we are thinking of
the following entries separately for each experimental day:

1. Date and indication whether seeded or not.
2. Precipitation recorded separately by each gage (snow
board) in the target and in control areas.
3. Identification of AgI generators operating on the given day.
4. Morning and evening nuclei counts.
5. Photocopies of radiosonde charts.
6. Wind direction and velocity at levels you consider the
most relevant.
7. Such other data as you may have handy.

Professor Lewis Grant
Professor Paul Mielke

-2-

20 December 1967

Also we would like to have a good map of the experimental region with marked locations of AgI generators and of precipitation gages, both in the target and in the comparison areas.

6. The compilation of all the above may require quite some time and, to speed up matters, you may be willing to send us things gradually, beginning with the items that are already available. In particular, if you have the data relevant to the problem of seeding effects in the presence of "low" and "high" inversion and of similarly defined "low" and "high" isothermal layers -- see Table II in our Denver paper, we shall be most grateful to receive them at your early convenience.

With repeated thanks and best wishes for a Merry Christmas and a Happy New Year.

Cordially,

J. Neyman

E. L. Scott

JN/jg

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

January 3, 1968

Dr. J. Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

A copy of my letter to Dr. Wyckoff is attached. I left for our Climax area immediately after my final was graded and didn't make it back to the office again until January 1.

We found your visit very stimulating and with the start of the new work year can get underway with some of the things we discussed. We will keep in contact with you and will start getting geared up to provide you with portions of the project data as they become available. (

Sincerely yours,

Lewis O. Grant
Lewis O. Grant
Associate Professor

LOG/dw

Enclosure

COLORADO STATE UNIVERSITY

-21-

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

January 3, 1968

Dr. P. H. Wyckoff
Program Director for Weather Modification
National Science Foundation
Washington, D. C. 20550

Dear Pete:

This letter is to confirm and expand on our conversation relating to the establishment of a working arrangement between the statistics portions of our Climax project group and Drs. Neyman and Scott of the Berkeley statistical laboratories.

Our Climax experiment has now been underway for seven years, first on a very limited scale, but on an expanded basis the past three years. This project involves both physical investigations, and others that are essentially statistical in nature. Considering for now only the statistical investigation of precipitation, which represents a relatively moderate portion of the total effort, very interesting and, we believe significant information, is becoming available. We are now well along in the interpretation of these data. Portions of our analyses have been released at the Fifth Berkeley Statistical Symposium; additional results will be presented at the AMS Albany meeting on Weather Modification, while still further results are being prepared for submission to the Journal of Meteorology.

The analyses are starting to isolate some of the meteorological conditions under which weather modification has varying effects. It is possible to make the analyses of effects under varying atmospheric conditions by stratifying the randomized data. This of course reduces the otherwise quite adequate sample to small samples for many of the important strata. A proposal is being prepared that includes the continuation of the randomized seeding. This proposal will describe in some detail the current status of the experiment and justification for continuation. One of the important justifications is the required increase of the sample size in the respective meteorologically controlled strata.

Dr. P. H. Wyckoff
January 3, 1968
Page 2

It is felt that this is the appropriate time to review the experimental design to modify it if this is desirable. The review would explore questions of the following nature:

1. Have increases in knowledge of statistical design in weather modification experiments been of a nature that would allow for improvements in the design in use by this project?
2. Are refinements possible that would increase the sensitivity for studying the seeding effects within an even broader range of meteorological variables?
3. Can the capability of determining differential results as a function of alternate treatment methods be incorporated?

Since such an experiment would run for several additional years, it is desirable to work with the most competent people available in considering any design modification. Drs. J. Neyman and Elizabeth Scott have expressed an interest in forming a cooperative effort in this statistical portion of the Climax experiment. Our CSU group and this Berkeley statistical laboratory group are preparing separate proposals for such a joint arrangement.

The efforts by the Berkeley statistical laboratory will be directed primarily toward refinement of design and development of statistical procedures for use in this, and consequently other, weather modification experiments. This will require working with and analyzing portions of the project data.

Since it will take close to a year to adequately consider modification in design and since it would be desirable to implement the modification for the 1968-69 winter season we would like to explore the possibility that NSF Grant No. GA-847 could be increased by \$5,000 during the remainder of this grant to permit the Berkeley Laboratory to get started on the program. This increased funding would provide for sub-contract funding to the Berkeley group. Subsequently, funding to that group would be on the basis of proposals submitted directly by that laboratory through regular NSF proposal channels. Their

Dr. P. H. Wyckoff
January 3, 1968
Page 3

proposal would be for the direct support starting in April of this year. The \$5,000 increase would be intended to carry the work to that date.

Your consideration of this matter will be appreciated.

Sincerely yours,

Lewis O. Grant
Associate Professor

LOG/dw

cc Drs. Neyman and Scott

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

January 12, 1968

Dr. Jerzy Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

Paul returned from his short trip to South Dakota on Wednesday and we were able to get together on many items on Thursday afternoon.

One item, of course, was our schedule for visiting you and Dr. Scott in Berkeley. Due to many complexities we would like to proposed two alternatives. A person is a slave to a field program when it is in operation. Despite one's plans, items constantly come up that have to have immediate attention. As you know our primary field operational season is during the winter months and I am highly committed to its proper functioning. In addition I am working to tight deadlines on the proposal to NSF, a paper for the Albany meeting with a February 1 deadline, a paper for a Bureau of Reclamation Planning Conference on February 15, and a paper for the International Cloud Physics Conference in Montreal, with a March 31 deadline. Paul, who has a heavy teaching load this quarter, and I are also trying to wind up our paper on the statistical analysis for the AMS Journal. Consequently, I would like to propose the following alternates for our visit:

Alternate #1: Paul could come out either the last weekend of January or the first or second weekend of February. I would plan to visit with you sometime after April when our winter field program is completed.

Alternate #2: Paul and I would come out together after April when the field program closes down.

Dr. Jerzy Neyman
January 12, 1968
Page 2

Please let us know how this would fit with your plans. It would probably be best if you would let Paul know directly as I will probably be in the mountains on a number of days the next several weeks.

We also discussed the compilation of data for you. It, of course, would be easy to supply you with a listing of experimental days and days of seeding. I passed out such a listing at the Berkeley meeting and will get it brought up to date and send it to you. It will not be too helpful for most uses though, since one needs all of the associated data (precipitation and meteorological). As you know from your visit this is difficult to assemble in a simple form. Our people are compiling, checking and putting it onto punch cards. This isn't going too steadily now while we are pulling them off for problems associated with current operations and special analyses for the papers with deadlines. And, of course, all of our tabulation help is comprised of students involved in course work who spend only parttime working during the school year. It appears to us that the best procedure would be for you to outline with us a specific problem analysis when we get together next time. The precise data requirements could be specified such that our data reduction students could do just what's needed and our programmer could write a program to pull the required data from the record sheets and tapes in just the form that you require. This seems more practical than trying to massively try to get everything together at once. We should go over this in detail when we get together again.

One last item, we will appreciate your assistance with the University of California Press in getting reprints of our Berkeley Symposium paper. A number of people have requested reprints that we have not been able to supply.

I have not heard from NSF. I assume that the matter is under consideration. I will let you know immediately upon receipt of any action.

Sincerely yours,

Lewis O. Grant
Lewis O. Grant
Associate Professor

LOG/dw

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

15 January 1968

Professor Paul Mielke
Mathematics Department
Colorado State University
Fort Collins, Colorado 80521

Dear Dr. Mielke:

This is in reply to the letter dated January 12th, just received from Professor Grant. As you must know, Professor Grant requested that I write directly to you. Naturally, we understand very well all the complexities of the academic life and sympathize in your difficulties.

We would like to speed up matters as much as convenient and would welcome your visit to Berkeley. Later on, of course, we shall be delighted to see Professor Grant. Regretfully, on the last week end of this month there will be a mathematical meeting in San Francisco and this may require our attention. Therefore, could we expect you for the first week end in February? Please let me know when you will be coming and on what nights you will want reservations. If you come in the evening, we shall meet you. Hopefully, there will be vacant rooms on those nights in the Faculty Club. Otherwise, we shall arrange for a room in a hotel.

I wonder whether the plan of supplying us with some data on your project could be speeded up. You must have certain things ready, presumably on punch cards, and it should not be difficult to produce a copy deck. To begin with, we would highly appreciate a list of data or, preferably, a deck of cards, with the following entries.

Professor Paul Mielke

-2-

16 January 1968

Experimental day	Date	Seeded or not	Average Precipitation in whatever units, inches or whatever		
			In target	In control Area 1	In Control Area 2
(1)	(2)	(3)	(4)	(5)	(6)
1					
2					
3					
4					
5					
6					
7					
8	1				
9					
10					

Presumably only columns (4)-(6) require comments. I am aware that you have many "sensors of precipitation" (gages, snow boards, etc.) both in your target and in the two control areas. If and when the NSF provides us with funds, we shall be interested in single observations. As of now, we could use the average amounts of the measurements of precipitation recorded by the particular instrument you think is the most reliable - the average being taken over all the instruments in the given area, that is in the target and in the two controls. Judging from the end paragraph of your Berkeley paper such averages must have been computed, at least for the target. Could we have them?

If, in addition to the above, you also have average counts of nuclei active at a fixed temperature, those taken in the morning and those taken in the afternoon, we would welcome them also.

Professor Grant requests information about the reprints of your paper. This is an item of our continuing conversations with the U.C. Press. In general, they are reluctant to provide reprints because they fear that their circulation will decrease the sales of the volume.

Professor Paul Mielke

-3-

16 January 1960

However, after some struggle, we succeeded in getting reprints for all the authors on condition that they will be delivered with a delay after the publication of the Proceedings (incidentally, this, of course, is the policy of most journals!). It is my understanding that the reprints will be shipped to the authors some time this month. I fully understand that the delay is annoying, but am not in the position to help.

In the meantime-- what about the day-by-day average precipitation amounts as requested above?

Looking forward to the pleasure of seeing you in Berkeley.

Sincerely yours,

J. Neyman

JN/jg

cc: Professor L. O. Grant

COLORADO STATE UNIVERSITY

-29-

STATISTICAL LABORATORY

FORT COLLINS, COLORADO 80521

STATISTICAL SERVICES

CONSULTING SERVICES

January 23, 1968

Professor Jerzy Neyman
Statistical Laboratory
Department of Statistics
University of California
Berkeley, California 94720

Dear Jerzy:

My present plans call for a trip to San Francisco during the first weekend of February. My airline reservations are as follows:

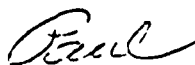
Arrive at San Francisco on United 265 at 7:48 p.m. on February 2
Depart From San Francisco on United 176 at 2:15 p.m. on February 4

I will appreciate having reservations made for Friday and Saturday evenings. Please let me know in advance as to where the reservation is made so I can arrange my transportation accordingly.

Lew has the request for information which you sent. He has been burdened with a multitude of high priorities lately such as the N.S.F. grant (which was finally submitted three weeks late) and some papers with deadlines in addition to the usual routine of academic life.

I'll be looking forward to seeing you and Betty in the near future.

Sincerely,



Paul W. Mielke, Jr.
Associate Professor

PWM/kd

UNIVERSITY OF CALIFORNIA, BERKELEY

- 30 -

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS

BERKELEY, CALIFORNIA 94720

January 26, 1968

Dr. Paul W. Mielke, Jr.
Statistical Laboratory
Colorado State University
Fort Collins, Colorado 80521

Dear Paul:

Because Jerry is out of town on a business trip, I am answering your letter of January 23. You now have a reservation for February 2 and 3 in the Durant Hotel near campus for a single room with bath.

We will meet you at the San Francisco Airport Friday, February 2 at 7:48 p.m. We are looking forward to seeing you then.

Sincerely yours,

Elizabeth L. Scott

bg

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

January 25, 1968

Dr. Jerzy Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

This is just a short note to let you know that during a conversation yesterday with Dr. Wyckoff of NSF on another subject, he mentioned that the \$5,000 had been approved and was being processed through administrative channels.

Sincerely yours,

L. O. Grant/dw

Lewis O. Grant
Associate Professor

LOG/dw

DBNR

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO



SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

29 January 1968

Professor Lewis O. Grant
Department of Atmospheric Science
Colorado State University
Fort Collins, Colorado 80521

Dear Professor Grant:

Thank you for your note with the welcome news about the \$5,000.

I interpret this to mean that the National Science Foundation approves the idea of a cooperation between us. This being the case we will begin to think about particulars regarding our own application. I will keep you informed.

We expect D. McNeve this coming week end and most sincerely hope that he will bring us at least some of the data -- without which we cannot do much. He is the youngest and huckiest of all of us and there are no limits to what youth can do. So, burdened as he is with lectures, etc., I suggest that you press him a little to collect at least such things as you have ready, except for preparing copies -- there must be plenty.

Cordially,

J. Neyman

JN/jg

February 9, 1963

Professor Lewis O. Grant
Department of Atmospheric Science
Colorado State University
Fort Collins, Colorado 80521

Professor Paul W. Mielke
Department of Mathematics and Statistics
Colorado State University
Fort Collins, Colorado 80521

Gentlemen:

This is to express pleasure at Professor Mielke's visit last week and to summarize the impressions I got from the examination of the sheet of data he left with us.

Regretfully, the data are not usable for any study of some reasonable scope.

During our visit at Fort Collins, one of the questions mentioned for the proposed co-operative study was that of spacing of precipitation sensors in the target. This question is partly empirical and can only be treated when precipitation amounts from each of the several target sensors are available. Contrary to this, all we received is the record of a single sensor.

We personally are interested in the relationship of effects of seeding and certain features of the atmospheric structure. The study of this problem requires the precipitation amounts and also the dates. Unfortunately, the sheet left with us by Professor Mielke does not give the dates on which the given snowfall occurred. Do you keep your precipitation records without dates?

Also, the same sheet does not give the precipitation amounts for all the experimental days. For some days the actual

Professors Grant and Mielke 2

February 9, 1968

precipitation amounts are replaced by dummy entries 9.990.
Professor Mielke's explanation was that on these particular
days there was some contamination by commercial seeding. While
this may have been the case, the precipitation amounts on the
days in question are still interesting.

The above circumstances, and also some remarks of Professor
Mielke, raise the question of the range of our proposed co-operation
as you see it. As far as we are concerned, this co-operation is
worth trying and may be fruitful only if we have uninhibited
access to all the data that are readily available (dates of
experimental days are certainly available!). As things stand
now, an application from us to the N.S.F. for support of this
co-operation can hardly be justified.

May we have your comments on these points?

Sincerely,

J. Neyman

E. L. Scott

bg

COLORADO STATE UNIVERSITY

-35-

STATISTICAL LABORATORY

FORT COLLINS, COLORADO 80521

STATISTICAL SERVICES

CONSULTING SERVICES

February 12, 1968

Professor Jerzy Neyman
Statistical Laboratory
Department of Statistics
University of California
Berkeley, California 94720

Dear Jerzy:

This is just a note to thank you and Betty for my delightful stay in California this previous weekend.

Incidentally, in a discussion since then with Lew, we plan to send you (as stated out in Berkeley) a documented tape containing the majority of our data.

Best wishes to the both of you.

Sincerely,



Paul W. Mielke, Jr.
Associate Professor

PWM/lh

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

February 13, 1968

Dr. Jerzy Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

Thanks for your February 9 letter which I just received. I will be participating in the Third Skywater Conference the rest of this week, so will have no chance to draft a detailed response until early next week. I do sympathize with your desire to get some data. As mentioned in my earlier letters, I have been 200% committed since mid-December. The commitment will drop to only 150% after this conference, and perhaps we can make some progress.

Sincerely yours,


Lewis O. Grant
Associate Professor

LOG/dw

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

February 21, 1968

Dr. Jerzy Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

Mr. Grant has asked me to drop you a note informing you that he is in Climax this week and will answer your letter after he gets back.

Sincerely yours,



(Miss) Dawn L. Woltemath
Secretary to Mr. Grant

dw

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

March 1, 1968

Dr. Jerzy Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

Please excuse this delayed response to your February 9 letter. I attended the Third Skywater Conference immediately after receipt of your letter. After the conference, I went to Climax. We had a fabulous storm and got some excellent data.

Apparently there is some misunderstanding on the "question of the range of our proposed cooperation." We certainly hope the cooperation can be maximized. My understanding of our cooperative arrangement is that outlined in my January 3 letter to Dr. Wyckoff:

" The efforts by the Berkeley statistical laboratory will be directed primarily toward refinement of design and development of statistical procedures for use in this, and consequently other, weather modification experiments. This will require working with and analyzing portions of the project data. "

You saw this in rough draft form when you were here in December and also received a copy of the letter to Dr. Wyckoff. Perhaps the problem has to do with timing. This is not really mentioned in this statement. We are obviously more restricted in this respect than you are. During the school year our primary program has got to be the operation of our field program, superimposed upon our academic responsibilities. While it's stretching things to the absolute limit, we find it essential to also work on some papers and attend certain conferences. Neither myself or my group can possibly fit in any more.

Dr. Jerzy Neyman
March 1, 1968
Page 2

I had assumed that you would be concentrating on basic aspects of statistical design that would explore questions such as those we discussed and as are also included in my January 3 letter:

1. Have increases in knowledge of statistical design in weather modification experiments been of a nature that would allow for improvements in the design in use by this project?
2. Are refinements possible that would increase the sensitivity for studying the seeding effects within an even broader range of meteorological variables?
3. Can the capability of determining differential results as a function of alternate treatment methods be incorporated?

We are cautious (perhaps unduly) in letting masses of data out until it is thoroughly checked. I would like to point out here that we have not used the general snowboard data from the total network yet in any of our own papers. We have restricted our publications to date to data from the permanently manned stations at HAO. There are several reasons for this. We are having separate conversions of all data from weight to equivalent inches of water depth made independently by at least two and in some cases three individuals. We are evaluating the quality of each observing site (certainly some sites are much better than others). We are comparing snowboard data (7 years) with corresponding shielded precipitation gages at the respective sites (2 years only). We want to make certain that this is done without knowledge of whether seeding was carried out. We also want to assure that procedures are consistent throughout the period. And last but not least, we would like to have the opportunity to report and publish our own data before it is released elsewhere. 11

A good example of the types of problems that exist are the 9.990 entries on the data that Paul gave you. We have identified these dates from reports to the State of Colorado as having been seeded by commercial firms. The records, however, were not adequate to tell whether the location or timing could have effected our area. Roger Reinking, one of my graduate students, has been working for over a year to run down the detailed information. We just received

Dr. Jerzy Neyman
March 1, 1968
Page 3

what we hope will be the last of the necessary information from Dr. Krick's group last week.

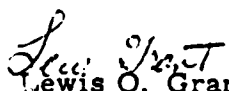
In the case of the ice nuclei data, we want to know how observations from the type counter we use compares with those from other counters. We also want to know something about what the counter observes in relation to what takes place within the cloud itself. We have concentrated very heavily during the past couple years in getting answers to these questions. We do not feel justified in releasing a lot of these data until some of these questions are resolved and until we have an opportunity to make at least some of these analyses of our own data.

I hope that the above clarifies what must seem like interminable delays in releasing data. The data processing itself has been almost at a standstill the past six weeks due to classroom schedules of my students and my personal requirements for data in completing committed papers. I will commit the following items to be mailed to you by Friday of next week:

1. A list of the randomized seeding dates
2. The precipitation data from HAO for the period 1960-1965 (subsequent HAO data is in storage to serve as an independent data sample).

We will supply other data as we can we can fit it into our schedules, but can make no commitments at this time. I hope that my understanding of our primary area of cooperation (the refinement of design and development of statistical procedures for use in this, and consequently other, weather modification experiments) is correct.

Sincerely yours,


Lewis O. Grant
Associate Professor

LOG/dw

7 March 1968

Professor Lewis O. Grant
Department of Atmospheric Sciences
Colorado State University
Fort Collins, Colorado 80521

Dear Professor Grant:

Many thanks for your letter of March 1st clarifying the situation which we find bizarre.

1. On November 17, 1967, you took the initiative in proposing a cooperative study on the development and refinement of the design of your experiment to increase the chances of identifying the conditions in which the effects of seeding are different. Following our enthusiastic response, Dr. Scott and I visited you at Fort Collins and saw many interesting things.

2. During our visit you emphasized the desirability of beginning the work at an early date so that its results could be used in the fall of 1968. In this connection you arranged a telephone conversation with Dr. Wyckoff at the NSF and requested a \$5,000 increase in your grant which could be used to finance some work in Berkeley. Simultaneously, I was to prepare an application to the NSF for an extension of the study.

3. Your letter of January 25th informed me that the \$5,000 are forthcoming.

4. In the several letters I wrote to you, I kept emphasizing the need of observational data on which we could start working.

5. Early in February we had the pleasure of a visit of Professor Mielke who did bring us some data. Unfortunately, as pointed out in our letter of February 9, the data brought by Professor Mielke was not useable for any kind of research and did not justify our applying for a grant from the National Science Foundation.

Professor Lewis O. Grant

-2-

7 March 1968

6. In your present letter, of March 1st, you explain that the delay in providing us with the data is due to various causes and "last but not least" because you "would like to have the opportunity to report and publish our own data before it is released elsewhere." Also, you indicate that next Friday you expect to send us two items: the dates of randomized seeding and the precipitation data from just one station, the HAO. Do you really think, or do you expect anyone else to think, that these two items are sufficient for the study of "refinement of (your) design"...?

My own attitude is, and Dr. Scott agrees with me, that a cooperative study requires some spirit of cooperation and a degree of confidence that one of the "cooperators" will not misappropriate anything from the other "cooperator." Your present letter indicates the lack of these two conditions and makes the proposed cooperation impossible.

Therefore, by a night letter just sent, I requested that you abstain from sending us any data at all. Also, I wish to request that in the future you abstain from listing the Statistical Laboratory as having anything to do with your project. Because this has been done in the past and, particularly, because of the extra grant of \$5000 which was meant for a subcontract with us, a copy of this letter is being sent to the National Science Foundation.

Yours sincerely,

J. Neyman

JN/jg

cc: Dr. P. H. Wyckoff

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

March 11, 1968

Dr. Jerzy Neyman
University of California
Department of Statistics
Statistical Laboratory
Berkeley, California 94720

Dear Dr. Neyman:

I am sorry that our cooperative efforts on design for weather modification experiments has not worked to your satisfaction.

A copy of my letter to Dr. Wyckoff asking him to withdraw the \$5,000 and to delete the reference to the Berkeley Statistical Laboratory is attached.

Best personal regards.

Sincerely yours,

Lew Grant
Lewis O. Grant
Associate Professor

LOG/dw

Enclosure

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF ATMOSPHERIC SCIENCE

March 11, 1968

Dr. P. H. Wyckoff
Program Director for Weather Modification
National Science Foundation
Washington, D. C. 20550

Dear Dr. Wyckoff:

I assume that you have received the 7 March letter from Professor Neyman. This points out that our proposed cooperative efforts are not working out. The enclosed copy of my letter of March 1 to Professor Neyman explains the situation as we see it. Certainly a substantial part of the problem has been our inability to deliver substantial samples of reduced field data to Professor Neyman on a schedule suitable to him. I am hopeful, but have no reason to expect that this might be the case, that at some future date when we are further along in data reduction that we can again have some sort of cooperative arrangements with Professor Neyman.

We are sorry to have caused you the inconvenience of processing the \$5,000 increase in our grant for use to finance some work at Berkeley. Since the administrative processing on this has not been completed, I assume that you can cancel it directly in the NSF administrative offices.

The only reference to this cooperative effort is included in our proposal "Physical and Statistical Study of Rocky Mountain Orographic Clouds and Precipitation and Their Modification." The reference to this cooperative effort, last paragraph, page 16, should be deleted in your considerations of this proposal.

The present statistical design used at Climax is believed to be at least as strong as any now in use for the evaluation of weather modification efforts. It is felt, however, that this is an appropriate time to review the experimental design and to strengthen it, if practical. As outlined in the proposal the objectives of this review are to answer the following questions:

Dr. P. H. Wyckoff

March 11, 1968

Page 2

1. Have increases in knowledge of statistical design in weather modification experiments been of a nature that would allow for improvements in the design of this project?
2. Are refinements possible that would increase the sensitivity for studying the seeding effects within an even broader range of meteorological variables?
3. Can the capability of determining differential results as a function of alternate treatment be incorporated?

This review will proceed as scheduled in the proposal. We will also undoubtedly want to discuss the design with outside specialists. Hopefully, Drs. Neyman and Scott, for whom we have the highest respect, will consent, along with others, to review the design when it is completed. Of necessity, and as originally scheduled, most of the design considerations will be made during the summer season after the close of the school and field program.

Sincerely yours,

Lewis O. Grant
Associate Professor

LOG/dw

Enclosure

cc: J. Neyman

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY · DAVIS · IRVINE · LOS ANGELES · RIVERSIDE · SAN DIEGO · SAN FRANCISCO



SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF STATISTICS
STATISTICAL LABORATORY

BERKELEY, CALIFORNIA 94720

March 1, 1968

Dr. P. H. Wyckoff
Program Director for Weather
Modification
National Science Foundation
Washington, D. C. 20550

Dear Dr. Wyckoff:

This is to report that, regretfully, our proposed cooperation with Professor Grant and Professor Miles did not work out. The enclosed copy of my letter to Professor Grant will explain the situation as we see it. In particular, I wish to inform you that our application for a grant to finance this cooperation will not be forthcoming.

Yours sincerely,

J. Neyman

W. J.
Enclosure

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

March 13, 1968

Prof. Jerzy Neyman
Statistical Laboratory
University of California, Berkley
Berkley, California 94720

Dear Prof. Neyman:

Thank you for your letter of March 7, 1968, with a copy of your letter to Prof. Grant of Colorado State University. We are sorry that this cooperative effort did not work out, and we hope that means may be found in the future to encourage more active participation between statisticians and physical scientists in the design of weather modification experimentation.

Sincerely yours,

PH Wyckoff

Peter H. Wyckoff
Program Director
Weather Modification

cc: Prof. L. O. Grant
Dept. of Atmospheric Science
Colorado State University
Fort Collins, Colorado

L MED
-80